However, birds and lizards are bad subjects for experimenting upon with supposed poisons, and do not conclusively prove that they might not be poisonous, or perhaps even fatal, to man. But being very busy at the time, I had no opportunity to carry my experiments further.

The forked tongue continually playing in and out of the mouth like a serpent's, the snake-like hiss, and the bright colors, together with their aggressive disposition, are well calculated to excite the suspicions of the Arizona Indians, who are reputed to greatly fear and thoroughly believe in the extreme venomousness of this reptile.

When intent on going anywhere in particular, their gait changes from a dragging of the body along the ground to that which an alligator assumes under similar circumstances; i.e., the body is carried high on the legs, clear from the ground, and the tail carried rigid and in line with the body.

They showed a peculiar fondness for water. When placed in a large tank with sloping bottom, in one end of which was water, all would spend most of their time lying where it was about an inch deep. This appears strange when recalling the arid character of the plains that they inhabit.

Their rations consisted of raw hen's-eggs, one of which made a full meal for a good-sized individual, which would not appear to care to dine more than once in about four days. These were given whole to the larger ones, which, having gotten the egg fairly in their jaws, experienced no difficulty in breaking the shell. Their mode of eating is by running the tongue into the mass of the egg, drawing it into the mouth, repeating this in a very deliberate manner, and spending from twenty minutes to a half-hour on an egg.

Their ability to climb is considerable; quite out of keeping with their heavy, unwieldy appearance. A tolerably smooth stick, an inch in diameter, standing at an angle of about sixty degrees, is quite easily ascended.

Several of them laid eggs during August and September. These were 53 millimetres long by 26 millimetres in transverse diameter, were perfect ellipsoids, having a finely granulated, soft, tough, translucent skin or shell.

HENRY L. WARD.

Tambaya, D.F., Mex., Dec. 25.

Sections of Fossils.

IN *Science* for Nov. 18, Prof. Joseph F. James, in speaking of the production of sections of *Bryozoa* for microscopic examination, says, "I can quote no higher authority than Mr. Archibald Geikie (*Text-Book of Geology*, pp. 85–88, where elaborate directions are given for making rock sections; Professor Prestwich also considers it 'an expensive and tedious process,' *Geology*, i. p. 43) as to the tediousness of the process." The pertinence of these references immediately vanishes if a person take but the pains to look them up. In both it will be found that the authors have been referring to the making of slides of Plutonic and metamorphic rocks. Of course, any one knows that a limestone in which *Bryozoa* are usually embedded cuts far more readily than crystalline rocks. Now, with a little practice, a man can soon cut from six to ten slides of crystalline rocks in a day; and he can cut six times as many slides of calcareous *Bryozoa* in the same time, as I have often seen done by college students, not by lapidaries. An average of from forty to sixty slides a day certainly cannot be complained of. Of course, no one will deny that the use of the microscope in fine petrographical studies of crystalline rocks has become imperative. We are here referring to *Bryozoa*.

Feeling convinced, from my own study of the writings of these authors, that they had never expressed an opinion of this subject, least of all with special reference to the *Bryozoa*, I sought for further information. Under date of Dec. 10, Prof. Joseph Prestwich writes me, "The question you ask about the *Bryozoa* is quite beyond my knowledge. I have never studied the *Bryozoa*. In fact, there are very few persons in England who have studied them. We lost our great authority in my old friend Mr. George Busk." In a letter dated Dec. 8, Prof. Archibald Geikie writes, "The question you propose to me in your letter is really one to which I do not feel myself competent to give an answer. I haver never given special study to the *Bryozoa*, and I have nowhere ventured to publish an expression of opinion."

The sentence quoted from Professor James's article concludes with the following words: "nor a better one than Dr. Nicholson as to the uncertainty of the results." In my article of Nov. 4, I mentioned Prof. H. A. Nicholson as one of the leading men who first took a decided stand in favor of the prominent use of internal characters as a means of classification. Now, it would not be fair to construe the above sentence as meaning that Professor Nicholson's writings are themselves a manifest example of the viciousness of the methods pursued by the new school. It must mean, therefore, that Professor Nicholson does not believe in the use of these microscopic sections. Since we interpret the spirit of Professor Nicholson's 'Genus Monticulipora' (1881) and 'Tabulate Corals' (1879) so differently, it will certainly be fair to quote his later writings, since they at the same time must contain his more mature views. Thus in the *Annals and Magazine of Natural History*, February, 1884, he writes, "The earlier observers of these fossils, as, for example, Mr. Lonsdale, necessarily founded their names upon macroscopic characters principally, the method of investigation by means of thin sections being of recent origin; and they also gave, as a rule, extremely brief descriptions. Hence it is exceedingly difficult, in many cases, among the monticuliporoids, to be certain as to the precise forms to which the older names should be attached." Then he proceeds to an investigation of both external and internal characteristics, accompanying the same with figures, of which those illustrating internal features alone are of value. In the number for December, 1885, he and Foord discuss the genus *Fistulipora* on the basis of the new light cast upon it by an investigation of the internal structure. Again in May, 1886, they make use of this method when they say, "Having recently had the opportunity of making a microscopical examination of a very extensive series of these forms, we have satisfied ourselves that they cannot be referred to the genus *Chætetes*, Fisher." And they propose the new genus *Rhaphidopora*. The plates 15, 16, and 17, accompanying this article, do not leave any doubt as to the position taken by these authors. The same is true of an article published by Nicholson and Etheridge in the same journal (March, 1886), where indeed they go so far as to separate *Stenopora australis* from *S. ovata*, with which "the specimens in question agreed entirely in external form and in macroscopic characters," solely on the basis of distinct internal features.

I cannot do better to express the opinions which actuate the new school of students than to quote from a letter from Prof. Archibald Geikie: "The common-sense view of such questions seems to me to be this. In dealing with fossils we are precluded in a vast number of cases from appealing to the evidence of internal structure, for it has not been preserved. Hence, if an organism can be satisfactorily determined from external characters, that is the most desirable means of identification, for it is the most generally applicable. If external characters are proved to be insufficient, and even misleading, we must fall back on internal structure when we can get it." Now, the new school believe that external characters often are misleading, where internal characters may more safely be followed. Since any *Bryozoa*, to be determined even according to the old method, must have the minute external structure well shown, and since in these cases the minute internal structure is also usually well preserved, we believe that the new method is eminently practicable. Nobody denies that external characters may be of great additional assistance.

AUG. F. FOERSTE.

Cambridge, Mass., Dec. 29.

Weather-Predicting.

IT has become a well-worn adage that half of the disputes would be avoided if the disputants had a thorough mutual understanding of the terms used by each. In weather predictions and verifications a clear understanding of the meaning of the terms used certainly seems very necessary. If a weather-predictor concludes that a satisfactory definition of a fair day is one on which less than .01 of an inch of rain falls, and a foul day is one on which more than .01 of an inch falls, and makes predictions accordingly, his predictions, when verified by this rule, will give a certain success in proportion to his skill. If, now, some one should object to cloudy days without rain being called fair, and record all cloudy days for which fair weather had been predicted as failures, he would give the predictions a much lower percentage of success

than by the first method. If he should go still further, and object to calling a day foul unless at least .05 of an inch of rain fell, and proceed to verify the above predictions accordingly, the percentage of success would rapidly approach zero. By disregarding this evident truth, Prof. H. A. Hazen has, in his letter on p. 322 of the last volume of *Science*, involved himself apparently in great confusion.

Mr. Rotch and the writer have during the last year published statements showing that local predictions issued from the Blue Hill Observatory for longer periods in advance than those issued by the Signal Service for this vicinity have had a higher percentage of success than the predictions of the latter. Some of these statements were copied in the notes of foreign meteorological journals, and were prominently referred to in an article by Dr. Klein.

In September, 1887, letters were received from Professor Hazen in which he referred to these statements, and said that our supposed higher success was 'all moonshine,' and was entirely due to our methods of verification. Moreover, he said it was unfair to verify predictions made for Massachusetts by the Boston record alone, and proposed that he and the writer should try together predicting for Boston alone. This seemed eminently fair, and the writer agreed to it; but, to make sure that both had a clear understanding of the meaning of the terms to be used, definitions of the terms 'fair weather,' etc., used by the writer in making predictions, published by the Associated Press of southern New England, were sent to Professor Hazen. He materially modified these, and sent the following definitions and rules. The temperature rules are omitted.

PLAN FOR WEATHER AND TEMPERATURE PREDICTIONS AND VERIFICATIONS AT BOSTON AND WASHINGTON (ALL VERIFICATIONS TO DEPEND ON THE OBSERVATIONS [TAKEN TRI-DAILY AT BOSTON]; PREDICTIONS TO BE MADE AT OR BEFORE 2 P.M., TO HOLD FROM MIDNIGHT TO MIDNIGHT).

Prediction: Fair Weather. — Successful: if fair three times; cloudy, fair, clear in any order; and any cloudiness less. Failure: if cloudy twice in any order; cloudy, fair, fair in any order, and any cloudiness above; a drop of rain.

Prediction: Threatening. — Success: if cloudy twice in any order; cloudy, fair, fair and any cloudiness above; rain .01 or less. Failure: if fair three times; cloudy, fair, clear in any order; and any cloudiness less; rain over .01.

Prediction: Rain. — Success: rain at any time over .01. Failure: rain .01 or less and any cloudiness.

Predictions were begun according to these rules, and the writer sent Professor Hazen a prediction during each day in October except on Sundays. Professor Hazen has correctly given these predictions, with the corresponding weather at Boston, on p. 323 of the last volume of *Science*. If any one will take these tables, and carefully verify the predictions in accordance with the above rules, he will find that sixteen of the predictions in Column 1, which represent the Blue Hill predictions, were verified, that is, sixty-four per cent of the whole; while only twelve of No. 2 (Professor Hazen's) were verified, or forty-eight per cent of the whole. This excess of sixteen per cent for Blue Hill apparently did not suit Professor Hazen, and he proceeds to obtain from Professors Russell and Upton other definitions and rules for verifying fair, threatening, and rainy weather; and, finding that these give a higher per cent for No. 2, he omits entirely to give his own rules. The writer likes Professor Upton's scheme better than that of Professor Hazen, only his predictions were not made in accordance with such a scheme. The predictions sent to Professor Hazen were not made to be verified in detail, but only to agree with his rules; and it so happened, that, while the writer was predicting with Professor Hazen, he was also predicting for the Boston papers; and when he predicted in these, "rain followed by fair weather," or *vice versa*, he merely wrote on Professor Hazen's card "rain," because, according to Professor Hazen's rules, any rain of over .01 of an inch was to be accounted success. Hence it is seen to be manifestly unfair to verify them by other rules.

According to the definitions sent to voluntary observers by the Signal Office, a fair day is one on which less than .01 of an inch of rain or snow (melted) fell, while a foul day is one on which .01 of an inch or more fell; and the writer was recently told by one of the predicting officers of the Signal Service that this was virtually the method used in the official verifications.

At Blue Hill this definition has been adopted, and hence the predictions are exactly comparable with those of the Signal Service. For October the Blue Hill predictions thus verified gave a percentage of success of eighty-five, while the Signal Service predictions only gave fifty-eight per cent for this vicinity. In both cases Sundays were omitted. Professor Hazen knew how this percentage was obtained, and yet in his letter to *Science* he writes as if it were a surprising thing that the same predictions should give eighty-five per cent when two things were considered, and only sixty-four per cent when three things were considered, in the verification.

H. HELM CLAYTON.

Blue Hill Observatory, Jan. 4.

American Microscopes.

IN my letter to *Science* (x. No. 252) in regard to American microscopes, I stated that my opinion in regard to them was based upon the examination of those brought to me by students. I hoped thus to avoid the appearance of claiming to have made an exhaustive examination of all forms of American microscopes. I regret that I did not make an express disclaimer.

Dr. Prudden has placed me under obligation by his very courteous letter in *Science* of Dec. 23, which calls attention to Grunow's new stands. Dr. Prudden's surmise that I was unaware of Grunow's recent work is correct. It is with much pleasure that I now learn that he is endeavoring to meet so admirably the demands of professional biologists and the needs of students.

Mr. Edward Bausch considers me unjust, if I do not misinterpret his letter (*Science*, Dec. 23). He appears to me to have overlooked that I wrote only in regard to microscopes suitable for biological, and particularly histological work. I have heard that the elaborate American stands were favorites with amateurs, but in regard to that point I expressed no opinion. I believe, however, that the increased demand for what is known as the continental stand is due to the rapid growth in numbers of those who use the microscope as a professional instrument, and to the extensive introduction of laboratory work in histology as a part of the course of instruction in our colleges and medical schools.

In regard to the Harvard microscope, Mr. Bausch may recollect, that, when he first came to consult me, I then urged upon him the advisability of frankly imitating one of the Zeiss stands. This advice he decided not to follow. At the time of his second visit I think that I again expressed to him the same advice. I also counselled him to make certain essential and some minor alterations. He made all of the latter, none of the former, if my memory is correct. He subsequently sent me a stand and two objectives to test. In reply I wrote the opinion which he has quoted in his letter, and which I see no occasion to alter now, but am compelled to append a remark for my own justification. The remark is, that I have since then examined a number of the Harvard microscopes brought to me by students. The stands have been of fairly good workmanship, but the objectives I have found, by conscientious examination, to be not infrequently of inferior quality, and most decidedly not satisfactory. As far, therefore, as my experience enables me to judge, I still feel disinclined to bestow the commendation upon these special American microscopes which I am ready to give to some of their foreign competitors.

My letter was not intended to impugn the honesty of the American manufacturers of microscopes, and I do not wish to do so at all. I do wish to call attention to the fact that their policy has been to supply instruments, which, however suitable for certain persons, are not as satisfactory for the work of the professional biologist, the medical practitioner, and of students, as are certain of the European microscopes.

It is to be hoped that Professor Ryder's interesting letter will bring about the result he suggests, of having a competent committee take up the consideration of the best attainable microscope. For my own part, I feel much pleased with a German stand of quite new model, which I purchased last summer. After using it a good deal, I have little change to wish for in it. If it should please others equally, it may be considered to represent an advance towards